with baize-covered tables for the students to sit at and read, and on the tables are fixed glass cases containing bones on which the attachment of muscles is clearly displayed. For the study of physiology an excellent laboratory and convenient lecture-room are provided, and by a simple arrangement of the partition between these they can be thrown into one, so as to make the whole of the side light available for microscopic work. The laboratory is supplied with an excellent collection of physiological apparatus, part of it being purchased by means of a donation from Dr. Henry Muirhead, the rest being the property of the lecturer. The chemistry department consists of a well-lighted and admirably fitted up lecture-room, the table having been specially constructed so as to give the lecturer a free supply of gas and water. Adjoining this is a laboratory capable of accommodating about forty students; it is quite a model of what such an establishment should be, the arrangements for gas, water, light, and test appliances being the best that could be devised. A new feature (as compared with other medical schools in this country) is a toxicological laboratory for teaching the testing of poisons. Much thought has been expended by the lecturer on medical jurisprudence in the arrangement of this department, and there is no doubt it will prove of much use as an aid to the study of an important branch of medical education. Near this is a room, at present unoccupied, but intended for a surgical museum and work-room in connexion with the surgery lectureship. A large and comfortable lecture-room for the other classes is situated on the ground floor, and lighted by an ingenious arrangement of roof and wall light, so as to secure a steady light without glare. Private rooms for the lecturers have been provided, and a private lavatory supplied with hot and cold water. One of the features of the school is a students' room, with lavatory attached; here, in the intervals of the classes, the students may smoke, read, or chat. It will no doubt be much appreciated by those for whose comfort it is intended. These buildings were formally opened and inaugurated on Wednesday evening, Nov. 1st, by a brilliant conversation, which was very well attended by the medical profession in Glasgow and neighbourhood.

Correspondence.

"Audi alteram partem."

THE OPPOSITION OF THE SCOTTISH UNIVERSITIES TO MEDICAL LEGISLATION.

To the Editor of THE LANCET.

SIR,—Will you kindly publish the following letter of Dr. Gairdner's, in reference to his attitude towards proposals for legislation for the creation of three conjoint boards, in place of the nineteen existing bodies in Schedule A of the Medical Act.

Yours truly,

Highbury, December, 1882.

J. G. GLOVER.

"Dear Dr. Glover,—In the only detailed report I have seen of the recent deputation from the British Medical Association to Lord Carlingford and Mr. Mundella (*Scotsman*, Nov. 23rd), I find you referring to my name along with that of Professor Turner, and it would almost appear as if our supposed assent to the principle of a joint board was to be made a starting-point for the further assumption that the opinion of Scotland, or of the Scottish universities, might be reckoned upon in future as favourable to some modification of that principle. Were it not for this suggestion, which appears both in your own remarks and in the leading article in THE LANCET of to-day, I might be content to leave Professor Turner's opinion to be read in his separate memorandum (F) subjoined to the report of the Royal Commission, and my own in my evidence, which you will find not at all in accordance with the views you attribute to me, see especially Nos. 4759–62, 4765–68, 4770–86. It is not my object at present to argue the matter; but as Lord Carlingford and Mr. Mundella, as well as others, may have been misled, I will ask you to secure the insertion of this letter, along with any report that may appear in THE LANCET of the proceedings of the deputation.

"I am, Sir, yours, &c.,

"W. T. GAIRDNER.

"Nov. 25th, 1882."

* * The subject of Dr. Gairdner's letter is noticed in another column.—ED. L.

"PICRIC ACID AND SUGAR TESTING."

To the Editor of THE LANCET.

SIR,—It would be a waste of my time and of your space to discuss the question whether my discovery or Dr. Pavy's criticism bears the impress of having been the more "hastily communicated." Upon this comparatively unimportant question the following reply to Dr. Pavy's letter may perhaps assist your readers to form their own judgment.

Dr. Pavy's statement of his belief that it "would be an absolute misfortune for a more sensitive test than the cupric test to be introduced into general use," would have appeared to me quite unintelligible if I had not lately had the benefit of a few minutes' conversation with him, during which he expressed his fear that a test so sensitive as to reveal the presence of sugar in normal urine would cause practitioners to mistake a physiological condition for diabetes. But surely the true way to obviate so serious an error is not to endeavour to keep the profession in ignorance of the easily demonstrated fact that sugar is a normal constituent of the urine; but to make it generally known, and to supply them with the simplest and surest means of verifying it for themselves. But, says Dr. Pavy, the alkaline sulphides cause the same reaction as grape sugar with picric acid and potash. Granted.

The question, then, is, Are alkaline sulphides always, or often, present in normal urine? There is no known test for sugar which may not give a similar reaction with some other material than sugar. Dr. Pavy, in his treatise on Diabetes (p. 16) states that chloroform, uric acid, and cellulose may cause a precipitate of suboxide in Fehling's cupric solution. To be forewarned of these possible fallacies is to be forearmed against them. But Dr. Pavy, in his letter, goes on to state that "sulphides of ammonium and potassium are capable of being generated by boiling urine with potash." This, so far as regards normal urine, I am told by much better chemists than myself, is an improbability, and cannot be accepted on the mere *ipse dixit* even of so great an authority as Dr. Pavy. He further states "that a sulphide is actually produced by the action of potash upon healthy urine is shown by the well-known fallacy that Moore's or the liquor potassæ test is open to from the presence of a little lead." I confess that this statement surprises me not a little. It has often happened to me in boiling albuminous urine with liquor potassæ to find the liquid darkened, and a subsequent precipitate of sulphide of lead, the sulphur being a constituent part of the albumen and the lead an impurity of the potash; but I have never observed this change to occur in healthy urine; and since reading Dr. Pavy's letter I have repeated the experiment on normal urine with a negative result, while adding a solution of lead to the mixture.¹

I have also repeated Dr. Pavy's experiment, which consists in forming an alkaline sulphide by boiling white wool with caustic potash. The liquid gives the same colour as grape sugar with picric acid. The addition of equal parts of this solution to healthy urine caused no appreciable change in the reaction with picric acid and potash. Then the question arises if a sulphide exists in the urine in a state to cause the reaction with picric acid, and so to interfere with the test for sugar, can the sulphide be detected by any other test? Obviously it can. On adding a few drops of the above-mentioned white wool product to healthy urine, and boiling with the addition of a minute quantity of lead solution, the mixture was soon blackened by the resulting plumbic sulphide. Again, on adding a few drops of Fehling's cupric solution to the mixture of the sulphide from white wool with normal urine, there was an abundant precipitate of cupric sulphide. The conclusion, then, is that normal urine contains no sulphides, nor any material convertible into sulphides by boiling with caustic potash, and that if from any exceptional condition sulphides are present, they may readily be detected and separated by well-known and very simple methods.

In testing for sugar in albuminous urine, the albumen may readily be removed by boiling and subsequent filtration before applying the picric acid and potash test. That this pre-

¹ Dr. Wm. Roberts says: "It was never found that liquor potassæ containing lead produced a dark-brown colouration with non-albuminous urines, provided, of course, that they were sugar free" (*A Practical Treatise on Urinary and Renal Diseases*, 2nd edition, p. 178).

liminary treatment is sometimes necessary in testing for sugar in albuminous urine, even by Fehling's solution, is stated by an eminent writer, whose authority will not be questioned by Dr. Pavy.² No one who is acquainted with Dr. Pavy's ammoniated cupric test can doubt that it is an exact and admirable method of quantitative analysis; but it requires a more complicated apparatus and a greater amount of manipulative skill than are possessed by the majority of practitioners, who would gladly avail themselves of some simpler method, which, even if it were less scientifically accurate, might for every-day use be of more practical value. Such a quantitative test, notwithstanding Dr. Pavy's regretfully expressed unfavourable predictions, will probably result from the "happy accident" before referred to.

My son having had his attention directed to the question of alkaline sulphides in normal urine, has made some experiments, the results of which are recorded in the following letter.

I am, Sir, yours faithfully,

Savile-row, Dec. 5th, 1882.

GEORGE JOHNSON.

* * Want of space compels us to hold over the letter of Mr. Stillingfleet Johnson, referred to above.—ED. L.

PHTHISIS, ITS ETIOLOGY AND TREATMENT.

To the Editor of THE LANCET.

SIR,—May I beg you will have the kindness to afford me sufficient space in THE LANCET to make a few remarks on a form of treatment of phthisis which has suggested itself to me from the start-point of the researches of R. Koch of Berlin, in whose hands the experimental method has resulted in proving that tuberculosis is, in reality, a parasitic disease of the internal animal organs, and that the parasite is a bacillus distinguishable from all other bacilli by its microphytic and other features. In his letter to *The Times* of April 22nd last, the vast importance of this discovery was fully illustrated in his usual felicitous manner by Professor Tyndall; and reflecting on this discovery, and reasoning from analogy, the thought occurred to me that a means which in my hands had been found of signal advantage in the treatment of leprosy in India might, assuming the truth of Koch's discovery, be equally useful if applied to that of tubercle; for I had many years ago ceased to entertain the least doubt that the *fons et origo* of leprosy was a microphytic one, and therefore to be reached by a means having for its object the destruction of that low form of organic life. The space to which I am limited here will not admit of more than a brief *résumé* of the views I advanced in my communication to *The Times* of June 8th. Speaking of the light thus thrown on the intimate nature of tuberculosis through Koch's painstaking researches, and of the "enormous superficial unlikeness," urged by Professor Tyndall, that existed between the eruptive diseases and tuberculosis, I endeavoured to enforce the view that, although such a dissimilarity did appear great, it was not in reality nearly so marked as that which existed between them and leprosy—a disease which I affirmed must be included in a like category—dependent, like them, on the growth within the animal body of a disease-producing bacillus. Hence I was led to suggest that a means which I had found of signal efficacy in the treatment of leprosy should be had recourse to in that of the kindred malady tuberculosis, the agent employed being carbolic acid in intimate union with the vapour of water at a high temperature. Thus employed the acid is, so to speak, atomised, losing thereby much of its irritant properties, and being brought into contact with the body of the patient, confined in a suitable vapour-proof apparatus, cutaneous absorption necessarily follows, thus saturating every tissue of the body with an agent in no way injurious to life when thus employed, though destructive of the microphytic life from which the disease has its source. Were the treatment of disease by the cutaneous absorption of medicinal agents a novel suggestion, or one involving the negation of physiological law, valid objection might well be taken against its application to that of tuberculosis. But it is not so; other forms of dis-

ease of a severe and an intractable type have been, and still are, successfully treated through the medium of cutaneous absorption.

Coming, then, to the subject I had in view in this communication, it occurred to me, as the outcome of the thought I have given to Koch's discovery, that seeing it has now been proved that the bacillus of tubercle is not a mere epiphenomenon of tubercle, but its *vera causa*, one cannot fail to see in it the true source of those infective materials whose existence has hitherto been assumed by pathologists, and which, disseminated by means of the blood and lymph streams, call into being those structural lesions which characterise tubercle as occurring in man and the lower animals. In the presence therefore of such conclusive evidence, is it unreasonable, I would ask, to anticipate that, should the resources of nature and art, as now known to us, fail to supply us with an agent possessing properties adapted to the object of reaching and destroying the microphytic sources of tuberculosis in the deep-seated tissues of the body, such an agent may not yet be brought to light? That in carbolic acid we possess such an agent in the treatment of leprosy, it has been my endeavour to show, whilst at the same time suggesting its possible applicability to that of tubercle. But are our existing resources really so meagre as to leave us destitute of other therapeutic means fitted to subserve the teaching of pathology? Without presuming to answer such a question in the affirmative, it is yet incumbent on me to submit to the attention of the profession a means which I would suggest is worthy of an extended trial—I mean salicin. Our knowledge of the action of this medicinal agent on the animal body, and of its reactions in disease, remains much as it was when communicated to the profession through the researches of Senator, Lehmann, and Miller. No doubt, in more recent years, additional light has been let in on the subject by other workers whose conclusions on the *rationale* of the action of salicin are, I presume to think, certainly suggestive of its being made applicable as a curative means to that family of diseases whose intimate nature and dependence on pathogenic germs the experimental method has placed beyond the reach of cavil or doubt. It is not in salicin, in its simple state, that such a result can be looked for, but in those changes which it most assuredly undergoes while circulating in the blood and lymph streams, where, subjected to the influences of the animal chemistry, it is transformed into agents whose destructive energy on microphytic life none can question. That the resultant of those changes is an acid body endowed with such qualities, abundant proof is not far to seek, though this is not the place to adduce it. Till very recently it was generally believed that this acid was salicylic—a view which is no longer tenable if it be proved, as very recently affirmed by Dr. Stewart of Edinburgh, and resting on experiments made by himself, that the acid is, in reality, the carbolic. Let this be as it may, be it carbolic or salicylic acid, both agents really belong to a like category, and are both equally available in the treatment of disease of microphytic origin—the former by the method suggested in my letter to *The Times*, the latter by the internal use of salicin, both agents leading to a like result—the destruction of pathogenic germs. But it is in salicin alone, and not in its compounds, that we must look for the realisation of this object—in its compounds no doubt, but only such as are fashioned in nature's laboratory; and not in the chemist's. Because—and if space were permitted to me here it would be easy to show that the salicylates—comporting themselves as other neutral organic salts, undergo changes in the gastric fluids which involve the destruction of the specific properties of the acid base, while, at the same time, often reacting injuriously on the system by calling into existence symptoms which at times have led to fatal consequences. And, further, it is to be remembered that the success of the treatment is contingent alone on the persistent and the uninterrupted use of the salicin; conditions which will be the more easily fulfilled since experiments have shown, notably those of Dr. Sydney Ringer, that, other things being equal, this substance can be taken in indefinite quantities and without inconvenience, which would be impossible in the case of the salicylates, even were they much less irritating than experience has shown them to be. The use of salicin must therefore be persistent and continuous in order to balance, so to speak, the equally constant elimination by the kidneys of the compound acids which it forms in the blood and lymph streams; and this, not for the purpose of

² The Nature and Treatment of Diabetes, by F. W. Pavy, M.D., F.R.S., 2nd edition, p. 17.